

other with a pocket sextant and ice horizon, justify the conclusion that the terrestrial refraction was so abnormal, that the computation of the latitude necessitated the reversal of the sign of the dip; but that this state of things was local, and that the observations of another observer only ninety miles away would not be so affected, though the temperature and general conditions were in both cases practically the same. (See comparison between the corrections for refraction in the meridian altitudes of Nansen at his Farthest North, and Hansen on board the *Fram*, April 6, 1895).

Admitting to the full the truth and justice of the remark of your reviewer in connection with the observations taken during Nansen's sledge expedition, that "the fact that observations were taken at all is the strongest possible evidence that scientific zeal is compatible with the possession of remarkable courage," it must also be admitted that a comparison of these scientific results with those which Nansen obtained from the same times and altitudes proves that scientific zeal and the power of taking observations are also compatible with the inability to comprehend the very elementary fact that if the results of two or more observations differ widely from each other neither is trustworthy, and that geographical positions and condemnations of the work of such men as Julius Payer and Wyprecht cannot, and ought not, to be based upon them.

Should any student of practical nautical astronomy go to the trouble of making this comparison, he cannot, I think, fail to perceive at every step that, however painstaking Prof. Geelmuysen and his colleagues have been in their attempt to plot Nansen's route on his celebrated sledge journey, they have been compelled to ignore his own statements and his own workings, and while straining at the scientific gnat they have freely swallowed the practical camel. Dr. Nansen had led us to believe that the scientific results would explain and justify his already published results. It can be easily shown that one or the other is hopelessly wrong. They are totally irreconcilable. If it is for one moment admitted that Nansen had the opportunity and ability to work the common observation for longitude by chronometer, then Prof. Geelmuysen's primary hypothesis is unsound. If it is maintained that that hypothesis is even approximately correct, Nansen's own recorded results become ridiculous.

Turning from matters of fact to matters of opinion, two statements of great interest to explorers in high latitudes may be noticed. On p. 14 we are informed that one of the computers employed with advantage the difference of altitude near the prime vertical to determine the latitude. Now in low latitudes, where the change of altitude is rapid, say from 10' to 15' per minute of time, a result within five or ten miles of the truth is perfectly possible. In latitudes from 70° to 85° N., with altitudes changing at most 5' or 6' per minute of time, and affected by refraction abnormal in itself, and varying rapidly according to no well-defined law, the method entirely fails. If a chance observation appears to justify its use, the altitudes must be accidentally or miraculously correct.

The remark on lunars, p. 22, will strike experienced observers as exceedingly curious. "The results," says Prof. Geelmuysen, "are not satisfactory."

Table C., p. 44, shows that eight observations were taken at various times, and from them Greenwich Mean Time was determined with errors varying from 18 seconds to 2 minutes. On the assumption that the explorers might have been dependent on them, their positions would have been affected with a maximum error of 30' of longitude, or about four geographical miles. Let future explorers note this. It may safely be affirmed that these results will seldom be surpassed by men taking lunars under Arctic conditions. It may with equal truth be said that for the purposes of such explorers greater accuracy is unnecessary; and the submission to a practical test of Prof. Geelmuysen's opinion, that better results can be obtained by deducing the moon's right ascension from the difference of azimuth of the moon and a star, will be a task not unworthy of the scientific expert accompanying the *Discovery*. E. PLUMSTEAD.

"First on the Antarctic Continent."

SOME rather venomous criticism of my book, "First on the Antarctic Continent," has appeared in one or two periodicals. Had my book been intended to be what it is not—a scientific report upon our work in the south—the venom would to some extent be justified. There are, however, other circumstances

which prevented me from producing at the time a larger and more representative account of our work in the south. Preliminarily may I state that the observations have been submitted to the Council of the Royal Society, who have accepted them, and the Society is in due time going to publish a volume on the results? This speaks for itself of the efficiency of the staff I had chosen. The Natural History Museum of South Kensington has received the bulk of the collections and I understand that the report upon them is nearly finished, and the book, written by specialists of the Museum, will probably appear within a very short time.

C. E. BORCHGREVINK.
(Commander, British Antarctic Expedition, 1898-1900).

Douglas Lodge, Bromley, Kent, July 5.

The Settlement of Solid Matter in Fresh and Salt Water.

IN a letter under the above heading in your issue of June 20, Mr. W. H. Wheeler discusses the effect of dissolved salt in promoting the subsidence of alluvial matter in water. He takes exception to the conclusion of Mr. Slidell that the mixture of sea water with river water exercises a preponderating influence on the formation of deltas. The question at issue is not one that can be settled simply by a consideration of the specific gravity and viscosity of the solutions employed, and Mr. Wheeler has made it the subject of experimental investigation. There can be little doubt that it is only in the case of very finely divided solid matter in suspension that the addition of salt solution causes increased precipitation, and so far his results can scarcely be called into question. They are confirmed by the investigations on the deposition of sediment by Carl Barus and Bodländer, to whose papers references are given below.

The precipitation of such "suspensions" or "pseudo-solutions" by the addition of an electrolyte is accompanied by the coagulation or flocculation of the solid matter. Schloëssing states that clay suspensions pass through a filter paper, but can easily be filtered if coagulated by a salt solution. If, however, the clay is washed free from salt, it can enter into suspension again in pure water and be precipitated afresh. These two operations can be performed in succession several times without apparent modification in the results. Picton and Linder found that the coagulum produced by the precipitation of a pseudo-solution of arsenic sulphide contained traces of the metallic ion, which could not be removed by washing.

The mud or ooze examined by Mr. Wheeler seems to have consisted entirely of matter which had already undergone precipitation, but it does not appear from his letter that any precautions were taken to remove traces of the metallic salts, so that it remains doubtful whether the sample really formed a suspension in the pure water. More satisfactory experiments could perhaps be made by collecting samples of turbid water from a river in flood and then adding sea water or a solution of salt.

I had occasion some time ago to consult the somewhat extensive literature dealing with the suspension of solid matter in a fluid and the allied one of colloidal solutions, and the following list of papers, though doubtless far from complete, may be of use to some readers of NATURE:—

Skey, *Chem. News*, xvii. p. 160; Waldie, *Chem. News*, July 24, 1874; *Journ. As. Soc. Bengal*, 1873; Th. Scheerer, *Pogg. Ann.*, lxxxii. p. 419, 1851, einige Beobachtungen über das absetzen auf geschwemmter pulverförmiger Körper in Flüssigkeiten; Hunt, *Proc. Bost. Soc. Nat. Hist.*, pp. 302-4, 1874; Slidell, *Report of Messrs. Humphreys and Abbott on the physics and hydraulics of the Mississippi*, App. A, p. 11, 1861; Ch. Schloëssing, *Compt. rend.*, lxx. p. 1345, 1870, sur la précipitation des limons par des solutions salines très-étendues; David Robertson, *Glasgow Geol. Soc. Trans.*, iv. pp. 257-9, 1874; W. Durham, *Chem. News*, xxx. p. 57, 1874; *Chem. News*, xxxvii. pp. 47-8, 1878; *Proc. Roy. Phys. Soc. Edin.*, iv. pp. 46-50, 1874; W. H. Brewer, *Proc. Nat. Acad. Sci.*, 1883; *Amer. Journ.* (3), xxix. p. 1, 1885; C. R. Stuntz, *Cincinnati Soc. Nat. Hist.*, Feb. 1886; E. W. Hilgard, *Amer. Journ.* vi., 1873, xvii., 1879, Forschungen auf d. Geb. d. Agriculturphysik von E. Wollny, ii. pp. 57-9, 441-454, 1879, ueber die Flockung kleiner Theilchen; A. Mayer, Forschungen auf d. Geb. d. Agriculturphysik von E. Wollny, ii. pp. 251-273; Hallock, *Bull. of the U.S. Geol. Survey*, xlii. p. 137, 1887; Carl Barus, *Bull. of the U.S. Geol.*

Survey, xxxvi. 1886, subsidence of fine solid particles in liquids, *Ann. Journ. Sci.* (3), xxxvii. p. 122; Carl Barus und E. A. Schneider, *Zeitschr. f. Physik. Chemie.*, viii. p. 285, 1891, über die Natur der colloidalen Lösungen; G. Bodländer, *Jahrb. f. Min.*, ii. pp. 147-168, 1893; *Götting. Nachr.*, p. 267, 1893, versuche über Suspensionen: Stanley Jevons, *Quart. Journ. Sci.*, viii. p. 167, 1878; Picton and Linder, *Chem. Soc. Journ.* lxi. pp. 114-172, 1892; lxvii. pp. 63-74, 1895; lxxi. pp. 568-573, 1897, solution and pseudo-solution; H. Schulz, *Journ. f. prakt. Chemie.*, xxv. p. 431, 1882; Hardy and Whetham, *Journ. of Physiology*, xxiv. p. 1899, *Phil. Mag.* Nov. 1899; Hardy, *Proc. Roy. Soc.*, lxvii. p. 95, p. 110, 1900; W. J. A. Bliss, *Phys. Review*, No. 11, 1895 (2).

H. S. ALLEN.

Blythwood Laboratory, Renfrew, N.B., June 27.

The Teaching of Mathematics.

BEING myself a teacher of mathematics, I have followed with much interest the vigorous crusade against the neglect of suitable scientific and mathematical training conducted by Prof. Perry and others, and am in substantial agreement with Prof. Minchin's remarks in his review in your columns of the series of papers by Prof. Perry on "England's Neglect of Science."

One thing has struck me in connection with school "mathematical" teaching as being a very illogical course of procedure on the part of the dominant "classical cleric" instructors of youth alluded to—namely, the teaching of *arithmetic*. A boy, whether classically or otherwise educated, is considered a dunce if he is not merely not an expert with the multiplication table, but even if he is unacquainted with such things as recurring decimals, square and cube roots, &c., whereas no attempt is generally made to give an insight into *theory*, the results, *i.e.* the *rules*, are what he is expected to know.

So dissociated to the ordinary mind is the science of arithmetic from mathematics that I can remember a fellow collegian actually remarking, "Mathematicians are bad at arithmetic"! It seems to me, on the other hand, that Euclid is much more out of the line of what we mean by mathematics. In teaching Euclid as a mathematical "subject," and, as some claim, as an introduction to geometry, we are actually raising barriers to the progress of a learner in grasping the meaning and uses of geometry. We insist on the propositions being learned *in all their cases*, insisting on the absolute distinctness of propositions which are merely particular cases of the same proposition, thus tacitly suggesting the existence of some such commandment as "Thou shalt not recognise the Principle of Continuity"—we ignore the infinite and we teach to try and wriggle away from the notion of a "limit." In fact, nearly all that really constitutes mathematics is carefully avoided in teaching of Euclid, whereas I have found, when I have dared once or twice to depart from examination ideals, how true the following remarks of Mr. C. Taylor in his prolegomena to "The Introduction to the Ancient and Modern Geometry of Conics" are. When referring to the work of Bosovich, he says:—"It is remarkable that Bosovich enters upon these abstruse speculations in an elementary treatise for beginners.... The preface to the volume contains an earnest plea for the introduction of the modern ideas into the schools. He had taught the appendix *vis à voce* to his own tyros with the happiest results.... demonstrations are put before him (the tyro) in an unsuggestive form which gives no play to his inventive faculty; and thus it comes to pass that of the many students so few turn out genuine geometers..."

I must not encroach further on your valuable space, although many points come to one's mind, such as the exclusion from so-called "higher algebra" papers of the theory of determinants, arithmetic without logarithms, applied mathematics without the calculus, &c., but, in hopes that the attack may be vigorously pushed home, subscribe myself yours sincerely,

Henry Smith School, Hartlepool.

F. L. WARD.

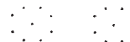
Curious Rain drops.

ON Thursday last, July 11, about 6 p.m., the day having been sultry, the sky became dark and overcast, threatening rain. Only a few scattered drops fell, however (the threatened rain passing off), but these sparse rain-drops drew my attention by their curious appearance on the sill of the window near which I sat.

Each rain-drop had broken up into a number of smaller drops,

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which arranged themselves in a circular form around a central one, in the manner here shown



Perhaps some one of your readers would kindly explain the cause of this, and if it was due to some electrical condition of the atmosphere.

M. S.

Bowdon, Cheshire, July 14.

THE MYCENÆAN QUESTION.¹

THE occasion for the following remarks on that difficult and much disputed subject, the Mycenæan Question, is furnished by the appearance of the timely volume on the "Oldest Civilization of Greece," by Mr. H. R. Hall, of the British Museum, and as public interest in the whole question has been considerably quickened by the important discoveries of Mr. A. J. Evans in Crete, this book, in which certain of the principal results of the Cretan excavations are discussed, will be heartily welcomed by the broad-minded school of classical archaeologists in general, and by the student of ancient Oriental civilisations in particular.

It is now some twenty-five years since the spade of Schliemann brought to light the remains of the oldest civilisation of Greece; and as it was soon recognised that these remains belonged to the period of the Bronze Age, it was clear that they must be older than the classical period of Greek culture. The excavations which were made subsequently in several parts of the Greek world by the various investigators who were emulating

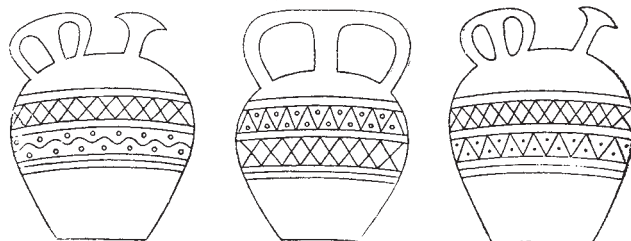


FIG. 1.—Representation of Mycenæan vases; from a fresco in the tomb of King Rameses III. at Thebes, B.C. 1200.

Schliemann's example proved that this Bronze Age culture was not confined to any particular part of Greece, but extended over the whole Hellenic area. Such discoveries compelled classical scholars to abandon many preconceived notions, and they found it necessary to revise entirely their ideas about the origins of Greek civilisation; it is not to be wondered at that many excellent scholars of the "old school" still find it difficult to make their views fall into line with the new order of things in classical archaeology. This is most evident when the dating of Mycenæan antiquities has to be considered, for if the Mycenæan culture, being of the Bronze Age, is necessarily pre-classical, its *floruit* must be assigned to the second millennium before Christ. An important confirmation of this view seems to be supplied by the evidence derived from the excavations which have been made in Egypt in recent years, where a large number of objects, pottery, &c., of Mycenæan origin have been found; and in many cases such objects have been discovered side by side with native Egyptian objects which must belong to the period which lies between B.C. 1500 and B.C. 1000. The discoveries of Mr. A. J. Evans, however, all seem to point to a still earlier date for the first development of

¹ "The Oldest Civilization of Greece: Studies of the Mycenæan Age." By H. R. Hall, M.A., Assistant in the Department of Egyptian and Assyrian Antiquities, British Museum. Pp. xxxiv + 346; with 76 illustrations. (London: D. Nutt, 1901.) Price 15s. net.